

*J. W. J. Merriam an*  
*from the Author*  
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OBSERVATIONS

ON THE

CONDITION OF THE BODY

AFTER

DEATH FROM CHOLERA.

BY

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*(Suggested by a Case communicated to the Writer by THOS. GREEN, M.D. F.R.C.S.  
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AN extreme apathy has, as might have been anticipated, long since succeeded to the intense excitement which prevailed whilst the cholera was raging; and there are many who may think it an injudicious moment to ask attention to anything respecting it; but I venture to address myself to those who, not behaving as though it were absolutely certain never to return, still contemplate its phenomena, and confess how much many of them need more research.

During October last I read an account in the *Times* of an "extraordinary case" of cholera, which happened at Bristol. It was described as "a supposed case of catalepsy," and subsequently as one of "suspended animation." It was reported to have "exhibited, perhaps, some of the most singular phenomena" on record. It was affirmed, too, that the subject of it had been heard to say, "that if he should die of cholera, he should like to be stabbed in the heart, so that he might be certain not to be buried alive." Rumours of every sort were spread on all sides: the public listened readily, as it always does,—was alarmed needlessly, as it often is.

The death which so many fancied to be simulated was, in truth, real all the while, as real as any death ever was, whether by cholera or other malady. An inquest was held upon the body after decomposition had become evident, and the fact of dissolution could be denied no longer. At this inquest "the utmost interest was manifested, and the jury-room was crowded, as well as the approaches to it." A great check must have been given to the appetite of the marvelling, by the Coroner observing that "the usual signs of death were well known, and, though some of these signs were wanting, it did not follow that death had not taken place."

The body had very long remained warm, and rigor mortis had been extremely tardy, and the Coroner re-

marked, on these points, that "the absence of coldness and rigidity frequently occurred, but there could be no doubt that death had taken place."\*

Dr. Wallis and Mr. Kelson, two of the medical attendants of the deceased, gave evidence during the inquiry which went to prove how little true cause had existed for the extravagant excitement which the case had occasioned; but at the same time there were circumstances stated by them of considerable medico-legal interest.

Desirous of the fullest particulars, I wrote to Dr. Green, Surgeon to the Bristol Infirmary, who also was consulted respecting the case, and he was so kind as to send me an account, which I have taken the liberty of abridging.

It appears that Mr. B. B——h went to his counting-house at 9 A.M., Oct. 12, 1849, and returned at 10, feeling ill, with severe pain, vomiting, and frequent watery evacuations. Some medicine and brandy were prescribed him.

At 4 o'clock there was great pain in the abdomen and legs; the voice was feeble; there was no pulse at the wrist; the face was pale and collapsed; the eyes half closed and sunken; the mind undisturbed. No urine was passed; there were vomiting and the characteristic rice-water evacuations; he would "roll in bed" with the intensity of the cramp. The tongue, however, felt warm; so did the skin and extremities.

A scruple of calomel was ordered to be taken, and a draught containing opium and ammonia, acetate of lead with small doses of morphia, external warmth, a mustard poultice to the whole abdomen, &c., were subsequently had recourse to.

When seen by Dr. Wallis, Dr. Green, and Mr. Kelson, at a later period of the day, there was a relief obtained from the vomiting, purging, and some from the cramps, whilst the skin kept warm; but still there was no pulse at the

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\* The words "*absence* of coldness and rigidity" are evidently intended to imply only unusually long *delay*.



wrist. Difficulty of breathing, with intense pain through the waist (spasm of the diaphragm?) was noticed.

When visited at 10 o'clock, this pain was relieved; the dyspnœa had ceased; there had been no return of the vomiting and purging; the skin was warm and perspiring; but there was scarcely, if any, pulse at the wrist: and so he continued until about half-past 1 o'clock in the morning (Oct. 13th), when, his bowels pressing for relief, he would get upright (in spite of the nurse, and contrary to what had been strictly enjoined him), passed a watery evacuation, and died in about twenty minutes.

"At 6 A.M., Oct. 13th," says Dr. Green, "I saw the body: the skin was warm; limbs not rigid; features not collapsed. I directed that he should not be buried until I had seen him again.

"At 6 P.M. I found he had been screwed down in his coffin: the lid was removed by my direction, the body taken out, and found still warm, and in the same condition as in the morning. The entire body and extremities were then closely packed in saw-dust

"Oct. 14th.—Had remained during the night in the saw-dust; the warmth still continued; there was no rigidity; a vein in the arm was opened, but no blood came.

"At 12 at noon, tepid salt and water were injected slowly in a vein of the arm; some frothing of the mouth followed; \* next powerful galvanic shocks were passed from the back of the neck in the direction of the heart for half an hour: no other means were used.

"The saw-dust was then removed, and the body laid out in the usual way.

"15th.—Heat less.

"16th.—Body cool.

"17th.—Body quite cold; limbs rigid; appearances of decomposition over the abdomen.—To be interred."

An account of the post-mortem examination was given at the inquest by Mr. Kelson: it presented nothing remarkable.

As to the measures which were resorted to with the apparent view of restoring animation, it will naturally be asked why they were not earlier employed? The answer is, that the me-

dical advisers never expected any good from them, and only used them to satisfy the friends of the deceased. They never doubted that the death was real, but were placed in difficulty by the unusual circumstances which surrounded it.

Dr. Green informs me that he considers the patient hastened his death by the effort to raise himself upright. Very likely. Many victims of cholera have suddenly perished in the moment of exertion, and the same may be said of other diseases which severely prostrate. In 1832 I was called to a man who lay in the collapse of cholera: he was cold, livid, dying. On a sudden, and without my being able to prevent him, he rose from his bed, would have fallen on the floor had I not supported him, and died in an instant. I could cite other like instances, were it necessary.

In the history before us there was nothing during life-time, as the late epidemic will fully prove, remarkable in any way; and as to what happened after death, there was nothing which should have set people marvelling to the extent they did, making them most anxious for the daily journals, that they might learn what had transpired in the "Bristol case" since the last report. But there were undoubtedly certain circumstances of no trivial interest to the scientific inquirer, which it may be well to dwell upon shortly, and the rather as a misconstruction of them may tend to the extension of erroneous views, and, perchance, at some future time, cause like events to be wrongly canvassed.

That a body dead on the 13th of October, and so early as half-past 1 in the morning, should not have been "quite cold" until the 17th, on which day rigor mortis seems to have first shown itself, and signs of putrefaction to have been first seen, is a fact which certainly requires comment. I could wish the task were in abler hands.

Now was there anything in the situation of the body subsequent to death which could account for any of the phenomena observed? Was the corpse covered or not?—exposed to a high or low temperature?—to a tolerably quiet or a quickly moving air? Was there aught to explain an uncommon appearance, or the absence of a common one?

There were four points in the corpse which principally attracted attention:—

1st. The long retention of warmth.

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\* This is commonly enough noticed in dead bodies on their being disturbed or moved.



2d. The deferred rigidity

3d. The delay of putrefaction.

4th. The aspect of the features, of which nothing has been said at present: it was placid, like that of the sleeper.

As to the first matter, it will doubtless have struck the reader that the packing of the body in saw-dust had, in every probability, much to do with the retention of the heat. It would be against reason not to conclude so; and such is the opinion of Dr. Green, Dr. Wallis, and Mr. Kelson; such that of Dr. Alfred Taylor and others to whom I have mentioned the facts of the case. Still, it seems clear that there was something not a little uncommon in the temperature of the body, and that the long-continued heat was far from being altogether owing to the bad non-conductor wherewith it was surrounded: the corpse, as we see by the account, had less heat on the 15th, was called "cool" on the 16th, and not pronounced quite cold until the 17th, though it was removed from the saw-dust about noon on the 14th. Inferring from Dr. Green's narration that the face was left uncovered whilst the rest of the body was completely enveloped in the saw-dust, I made inquiry of that gentleman whether the exposed part did not become more quickly cold than the rest of the body. Dr. Green says, in a note, "I took care that the face should be entirely uncovered. The next morning there was not much, if any, difference between the temperature of the face and other parts; but on the morning of the 15th the face was decidedly colder than the rest of the body." From this we may gather that the saw-dust operated in keeping, or helping to keep, the body warm: indeed, it could not have done otherwise; but the corpse appears to have been very slow in cooling even when there seemed nothing to prevent its doing so. I asked Dr. Green to inform me of the circumstances under which the body lay subsequently to its being removed from the saw-dust, of the temperature of the atmosphere meanwhile, &c., and I learn from him that it was laid on a board, dressed as the dead are usually, and covered with a sheet. No fire was in the room it lay in. The window was open in the day time, and shut at night. The following was the temperature of the weather at Bristol in four out of the five days wherein the corpse was watched:—

Saturday, Oct. 13th, 45°, wind East.

\*Monday „ 15th, 45° „ East.

Tuesday „ 16th, 47° „ E. S.E.

Wednesday 17th, 60° „ W. S.W.

From hence we may conclude that the body was slow in cooling even when by no means unfavourably situated for losing its warmth quickly, when the temperature was moderately low, the wind being in the east the while, when the window of the room was open at day-time, and the body lightly covered.

No doubt it would have been very instructive to have had some observations made carefully by the thermometer in the case of Mr. B——, setting forth plainly a true comparison between the heat of the body in life and death, and marking to a nicety the slow degrees by which it lingeringly left the body; but it is not surprising that they were not effected, seeing how every moment of the time of most practitioners was occupied during our late dreadful and memorable visitation of cholera, wherever the pestilence thickly raged. And I must be permitted to say, that it would be absurd to put aside all observations on the ground that they might have been exacter and more delicate. As a rule, we know by our touch when bodies are hot and cold; and in taking the temperature of patients under different circumstances, the thermometer is continually verifying what we have ascertained, though not with enough minuteness, by the hand, which, to test a matter of this kind, is confessedly an infinitely ruder instrument. Persons have felt the bodies of those dead by cholera, and given a *concurrent* testimony that they were warmer, greatly warmer, than in life-time, and surely we must admit their evidence notwithstanding no thermometer was used.† I know that some of the profession have been summoned to the dead by reason of their growing warm; they obeyed the call as one of the fancy, but were soon obliged to confess the existence of, to their minds, a strange and novel phenomenon.

In the case before us it is not stated

\* Sunday was not mentioned; but the temperature observed at Greenwich was, highest, 48°, wind N.E.; lowest, 37°, wind N. N.E.

† I presume in almost all cases where evidence is given upon inquests and trials, that where the warmth or coldness of the dead is mentioned by the medical witness, he refers to what he ascertained by the touch. The thermometer should certainly be oftener used.



that there was any *rise* of heat; the long duration of it was the striking circumstance. Perhaps the same phenomenon would have been seen in other instances of cholera had they been patiently, enduringly watched. But as far as my own experience goes I have nothing to contribute on this head. As to what usually happens after various deaths, Dr. A. Taylor says—"it is very rare that the heat of the body is preserved for so long a period as twenty hours. In general it becomes cold within ten or twelve."\* This observation, however, only applies, and is doubtless intended only to apply, to the external parts of the body. Organs and strictures imbedded deeply hold their warmth long after the surface, exposed perhaps to the cold air all but entirely, has parted with its heat, or with so much of it as to chill the hand of living persons. A little inquiry has told me that the thermometer often exhibits the presence of a degree of heat far higher than that of the surrounding atmosphere when the body strikes cold to the touch, and that it proceeds to lose it very gradually.

In inquiries into the remaining heat of the dead the relative temperature of the different parts should be accurately taken: it is not enough to examine the axilla, and leave the matter there. In a girl, aged 18, a sufferer from morbus Brightii, whereto succeeded diffuse bronchitis, coma, and death, I found, 24 hours after dissolution, the atmosphere being of the temperature of  $40^{\circ}$ , that the thermometer proved  $60^{\circ}$  of heat in the axilla, whilst  $72^{\circ}$  were found in the kidney. In another instance, wherein there was also renal degeneration, followed by a rapidly fatal apoplexy, there was a still more considerable difference between the heat of the kidney and the axilla. But let me refer to some remarks, by Dr. Davy, on the temperature of the body after death, in which the comparative amount remaining to various organs was minutely examined. The following is one of the instances which he cites:—

"—, aged 29; died of pulmonary consumption on the 26th January, and was examined eighteen hours after. There was great emaciation; little blood; there were the ordinary lesions

in the lungs, with empyema of left pleura, and ulceration of ileum and cæcum.

	Deg.
In longitudinal sinus (calvaria usually thin) . . . . .	55
In lateral ventricle . . . . .	55
In upper part of spinal canal . . . .	55
Under integuments of sole of foot . .	43
" " dorsum of foot . . . . .	44
" " thigh, close to artery . . . . .	52
In purulent fluid of left pleura (46 oz.)	64
In right ventricle, moderately distended with blood, partly liquid, which afterwards coagulated . . . .	63
In left ventricle, empty . . . . .	64
In central substance of right lobe of liver . . . . .	65

The observations were made in succession, without interruption, in the space of five minutes. The temperature of the room the greater part of the time was  $40^{\circ}$ .\* It appears from this and other observations that not only must the external and internal parts of the body be contrasted, but the latter compared one with the other. *Ceteris paribus*, the most exposed portions most rapidly cool. How could it be otherwise? Make the skin internal and the heart external, and the latter loses its heat the more quickly. The uncovered arm of the dead is soon frigid, the covered preserves its heat. The other day a man was brought into the Westminster Hospital completely dead. He had strangled himself with a small rope which had left the deep mark of its effectual compression. His ordinary dress was on him; the parts enveloped by it were warm, but the hands and other exposed portions of the body were quite cold. In the dying as in the dead we note frequently the effect of external cold, and the partial failure of animal heat is a not uncommon foreshadower of dissolution; the paralytic limb is warm or chilly, as it is laid open to the air or not; and the leg robbed of blood, though temporarily, by Hunter's operation shows the same fact. These are points known to all, but perhaps dwelt upon to the full by few. There is a multitude of occasions wherein the effect of exposure and covering must be well accounted for. *Imparted* must be distinguished from *self-generated* heat. A part will mortify despite the warmth

\* See his instructive and valuable Lectures on the Signs of Death, published in the MEDICAL GAZETTE. Vol. for 1837 and 1838.

\* See Researches, Physiological and Anatomical, by John Davy, M.D., F.R.S., Vol. i. p. 242.



that is not its own; it can be raised in temperature as dead matter may. There may be life with extreme coldness, death with much warmth. The questions are, what has led to the production of either state—transient or permanent? and how is it influenced in every way?

The temperature of the body after death by disease in general has been hitherto but very insufficiently considered; and the consequence is that we are but ill prepared to treat of the matter as respects cholera. Yet the subject is of too great interest to justify our ignorance, is no doubt intimately connected with the phenomena exhibited by the disease in life-time, and has, probably, far more numerous, various, and useful bearings than we should make bold to guess. But as far as the profession itself is concerned, it is certainly incumbent that it should acquire some knowledge of the question: it should be prepared, at any rate, to be more informed than those who may consult it in legal inquiry.

The most curious matter connected with the subject is not the *maintenance* of temperature, but its *rise*.

The editor of the Cholera Gazette for 1832, remarks, in a note, that the warmth of the body increases long and remarkably after death; to him it was clearly no rarity, and yet I could point to most able writers on the scourge referred to, who devote not a syllable to the matter, and could also mention some of my acquaintances who have had a very unusual experience of the disease, and have never observed the phenomenon.

Cruveilhier, in his famous work on pathology, speaks of the coldness in this disease being less intense in the dead than the living, stating:—"Le froid de la peau est moins intense sur le cadavre que sur le vivant, ce qui tient en grande partie à l'absence de cette sueur visqueuse qui inonde la peau pendant la dernière période du choléra: chez plusieurs sujets, au bout de dix-huit heures, la chaleur du tronc s'était maintenue à peu près comme dans l'asphyxie."

The circumstance of the heat thus augmenting, and being retained thus long, is certainly one which I think could hardly have been anticipated in a disease wherein in life-time there is so general and signal a failure of the animal warmth,—which in fact takes

rank amongst the most prominent and ominous features of this dreadful affliction. Strange, that the body which feels so cold in life, that we all but shrink from the icy chill of its impression, should, after the last pulse has beat, the last breath been taken, impart so much warmth to the toucher, alarm perhaps some anxious relative who, with senses made keen by affection, bends himself solicitously over the body, sure to discover anything unusual, and make the most of it when known.

What means this manifestation and retention of heat? With what is it associated? Is it connected with those singular disturbances of the muscular fibre whereby the face is agitated and the limbs moved? I have in a former essay referred to a case in which the heat rose and the muscles contracted. Dr. Taylor makes mention of another—and more, doubtless, have been seen—in which this elevation of the animal heat and marked signs of muscular irritability were beheld simultaneously; but there is no reason to believe that they appear together as any other than *coincident* events. The motion has often happened without the heat, the heat without the motion.

In the two following examples which occurred last year, under the notice of Mr. Hunt, who communicated them to me, there were no contractions noticed, though the rise of heat was palpable enough.

Ellen Connor, æt. 50, died at 49, Duck Lane, Westminster (a place full of dirty, crowded, ill-ventilated apologies for dwelling-houses, and in which the late epidemic raged frightfully,) of an attack of cholera, after being ill twenty hours; she was extremely cold in life-time, but after death was found much warmer than her son, who, stricken with the same malady, lay beside the corpse of his mother, and died three hours afterwards. Mr. Hunt was told by the relatives of the man, that his body, also, became warmer after life was past.

Jane Welch, æt. 7 years, died of the same disease, after an attack of 12 hours' duration. Mr. Hunt examined the body carefully in life-time, and it everywhere struck him with a "sudden chill." But after the child was dead two hours, he was sent for hastily by the parents, who were wishful to ask him if it were not living, for the body had become warm and the cheeks somewhat red-



dened. Mr. Hunt placed his hands upon the body and felt a "glow of warmth," and was for a moment inclined to hope, but soon convinced himself how irrecoverably the heart and respiratory muscles had ceased to act.

It cannot be questioned that very many instances of rise of temperature altogether escaped observation; first, because so many of the dead were not carefully examined; secondly, because the mode of examination by touch in some of the cases wherein it was employed, could not have measured those *slighter* elevations of the temperature which may be assumed to have occasionally happened. The extreme examples were no doubt detected readily; even the most ignorant could find them out.

Dr. Taylor refers to some instances in which the thermometer was used:—"In many cases of death from cholera in 1832-3, the body which had become moderately cold was observed suddenly to resume its warmth, so that the temperature is stated to have risen some time after death, as high as  $87^{\circ}$ , although the circulation and respiration had entirely ceased."

As to the cause of the *post-mortem* warmth, Cruveilhier, as we have seen, inclines much to refer it to the withdrawal of the cooling influence attendant upon the evaporation of moisture from the surface which took place in life-time. This view may be sometimes capable of explaining the lessened coldness to *some extent* in marked cases of increased heat, but not nearly to a degree that is satisfactory. Have any observations been made to show that those bodies which were most bedewed with cutaneous exhalation exhibited after death a higher temperature than others which were less moistened by it? But in reference to its refrigerating effect in life-time, we must remember how closely and thickly many of the bodies were carefully covered, so that a free evaporation from the surface, whereby heat could largely and quickly escape, was effectually prevented.

At the time of death by cholera there is an amount of internal heat which must be diffused and become external ere the body be cold throughout; but surely it cannot be successfully contended that enough remains to explain a very palpable rise and a much more than common retention of warmth. As

it is diffused so it is diminished; when it gains the surface it must be of less intensity than it was before. Other circumstances being equal, the warmer the body at the instant of death the longer does it take to cool: some lose their heat in great part before they die; not so others, certain of those, for instance, destroyed of a sudden by disease, accident, or purposed violence. In reference to the case which occurred in Bristol, it must be recollected that the patient was warm when he died, and further, that his dissolution was apparently hurried by a sudden effort: had he lived longer, so as more gradually to perish, it is very doubtful whether the heat of his body would have lingered so long as it did. The quicker the dissolution the more likely is the temperature to be long retained,—the more probable is it, also, that rigidity will be postponed, and decay retarded.

Since the rise of temperature which occasionally happens after death by cholera is found often in the absence of muscular contractions, it would be out of question to gravely refer thereto by way of accounting for the production of the heat. And were the heat never found except in combination with the movements of the muscles, it would still be futile to appeal to them to explain the cause of warmth. There is nothing in the observations of Becquerel and Breschet, who found that the temperature of a muscle increased  $2^{\circ}$  by violent exercise, which can show reason for any considerable augmentation and duration of heat. Besides, we must have a due care not to apply indiscriminately to the dead what has been discovered to happen in the living, as to change and amount of temperature. For animal heat bears a certain relation to the energy of the circulation, and the blood speeds more swiftly when the muscles play. But mere muscular action when no blood is circulating,—such, for example, as may be brought about by galvanism when life has left,—does not, in every probability, originate any thing like that increase of warmth which would be generated by motion under different circumstances. How cold the cholera-stricken are in life-time, despite those horrid and pertinacious cramps which violently, and sometimes almost universally, pervade the tortured frame!

Has putrefaction any concern in the production of the increased temperature



after fatal cholera? In some intestinal affections it is well known that putrefaction happens early: in cases of obstruction and puerperal fever decomposition has very hastily appeared; but in cholera I never remember to have noticed any unusually rapid signs of decomposition on the surface of the body. Cruveilhier asserts that "the putrefaction of the body is slow in this disease, as in all subjects drained of their blood; but that the putrefaction of the alimentary canal is, on the contrary, very rapid, as in all cases of considerable sanguineous congestion of the digestive organs." Generally speaking, the obvious putrefaction of the dead is delayed until they have become cold: they are warmer at the moment of dying than at any time subsequently. Did the rise of temperature after death from cholera begin later than it does, keep pace with advancing putrefaction, become most intense where this process was most manifest, there might be better grounds for speaking of decomposition as the cause of the heat than seem to exist at present. Every one knows, who has been much in the dissecting-room, that bodies in the very height of putrefaction will strike the hand with no sensible degree of warmth. The degree of heat whereto the body sometimes rises after death is a subject likely to puzzle the philosopher, and for the reason, partly, that we are rightly wont to connect augmentation of temperature with increased nervous energy and accelerated circulation. When we lay our hand upon the dead body, and find that it has become warm, we are naturally surprised at a circumstance so very unusual. "The power of generating heat," says Mr. Hunter, "seems to be a property in an animal while *alive*."\* But heat is, in some mode or other, generated in the dead sometimes, and, as I am persuaded, otherwise than by putrefaction. Chemical changes, of which the chief is the formation of carbonic acid, do, as it seems to me, go on in the body after dissolution, and give rise to that amount of temperature which is occasionally noticed.

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\* See Experiments and Observations on Animals with respect to the power of producing heat. Hunter's Works, Palmer's Edition, vol. iv. p. 131. Some of the many difficulties wherewith the subject is surrounded are most emphatically set forth, and the contribution abounds in the most invaluable suggestions as to further investigations.

Hitherto we have thought that the body of the dead when once cold invariably remains so, but we must remember how little, how superficially, the condition of corpses as to temperature has been investigated. In this, as in every other matter, the chief cause of ignorance has been deficient inquiry. Dr. Taylor observes, in reference to the state of the temperature of the dead by cholera—"It is scarcely to be imagined that the function of calorification should for an instant continue in the really dead; and yet certain facts connected with malignant cholera would almost seem to establish the possibility of this." But further on he says—"Possibly a sudden vital reaction in the nervous system may have given rise to the effect observed."\*

Mr. (now Dr.) Mayo remarks in his Outlines of Pathology†—"Magendie antithetically observed that Asiatic cholera begins where other diseases end—in death. And certainly many of its features impress us with the idea of some extraneous and peculiar force of prostration which extinguishes life; and—which is, indeed, most wonderful—seems to act upon life by depressing without exhausting the vitality of the organization. This language, I am aware, is too fanciful for sober physiology; but it is suggested by certain remarkable features of the disease yet unadverted to. It is well attested that in many cases of the most formidable type of cholera, when the patient for two or three hours before death has been cold, blue, motionless, the limbs stiffened with cramp,—upon death taking place the limbs and the jaws have begun to move in slow convulsions, the skin has become sensibly warmer, and the blue tinge has gone off, changing to a mottled red,—as if the organization, which had been borne down during the struggle between life and the disease, could temporarily re-exert its forces when the pressure which extinguished life had ceased."

But *what* "forces of the organization" are thus "temporarily re-exerted," and *how*? Is the heat, as to formation, simply similar to that of life-time, in being dependent on the like chemical combinations, though they take place under completely opposite circum-

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\* Vide op. cit.

† Vide p. 328.



stances? Supposing carbonic acid to be formed, whence comes the oxygen that combines with the carbon,—how are its elements brought together that they may unite? Does the oxygen exist within, or does it proceed from without, furnished in abundance by the air itself, as a distinguished animal chemist\* will be seen to suggest presently? Is the elevation of temperature *confined* to the *skin*, or is it diffused wherever there are vessels? Is it always accompanied by a change of colour on the surface, by that mottled red which Mr. Mayo mentions, and which I have had occasion to observe myself? How much, how very much, remains to be explored ere we can gain any satisfactory knowledge of this subject! The late epidemic, with its thousands of deaths, has scarcely taught us any thing about it.

Knowing that Dr. Garrod had made much inquiry into the state of the blood in cholera, I requested him to have the goodness to state his view of the most likely way in which he considered the rise of heat after death to be occasioned. As it will be better to lay his own words before the readers, I beg attention to a literal copy of a letter where-with I have just been obliged by him.

“63, Harley Street, April 1850.

“MY DEAR SIR,—You ask my opinion as to the probable cause of the increase of temperature observed frequently to occur in the bodies of patients who have died of cholera. I think, in the first place, that it is impossible that putrefaction should have any share in the production of the phenomenon, as the time at which it occurs shows; and also the fact that cholera bodies are not at all prone to decomposition. It must, therefore, be some other chemical change, and I conceive that the following must be the explanation;—During the collapsed stage of the disease, the blood, from imperfect circulation, &c., is prevented from being properly oxygenised, and hence a venous blood is circulating in all the vessels of the body, accounting for the cold stage. After death, however, the blood in the superficial capillaries becomes acted upon by the oxygen of the external air, and the formation of carbonic acid, with the production of heat, ensues: this heat

often for a time much exceeds in amount that which is abstracted by the cooling influence of the surrounding air: after death from most other causes, the blood, having become oxygenated during life, no further heat is evolved, and the ordinary cooling process ensues.

“I am, dear sir,

“Yours faithfully,

“A. B. GARROD.

“W. F. Barlow, Esq.”

It is unnecessary to ask the attention of the reader to these observations. That the oxygen of the air does act upon the blood in the capillaries may be inferred from the fact of the livid colour of the body giving way occasionally to a red tint. The same thing has been noted in the asphyxiated, and perhaps in these also some warmth may be generated, after the breathing has ceased, by the aerial oxygen acting on the carbon of the venous blood, where-with the superficial vessels are charged heavily. If the view suggested by Dr. Garrod be a right one, the skin would probably be found in such instances as he refers to (were a thermometer carefully and properly employed) warmer than the parts beneath.\*

It would be interesting to know whether the augmentation of heat be invariably accompanied by change of colour, and whether those bodies become most elevated in temperature which are most livid at the time of death. It would be of moment to know, also, the relative effects of exposing some parts to the air, and of keeping others, as far as possible, excluded from it. Changes of the blood in the capillary vessels *may* partly explain long *maintenance* of heat where the body is exposed to the air, and not in cholera only, but other cases. As the chemical combinations, on which the heat may be supposed dependent, are of quick or gradual completion, so will the warmth of surface hasten away or linger. We are supposing a case in which a high temperature, so far as mere *chemical* changes go, is produced *immediately* by the same causes in the living. Though oxygen be no more supplied from within by ever fresh and fastly-flowing currents of arterial blood, it may be obtained from without; but now it acts upon stirless,

\* It had occurred to Dr. Basham and myself some time ago, that a comparison of the heat of the external and deep-seated parts might tend to some important conclusions.



not moving blood, producing changes which, quite compatible with the most absolute death, serve no perceivable purpose. As the clot of blood drawn *from* the vessels reddens when exposed to the atmosphere, so does the still blood *in* them change colour when the surface of the body lies uncovered after death, for that which intervenes between it and the air suffices not to hinder the influence of the latter. The question is, not as to the fact of influence, but the extent and results of it.

The cause of the coldness of the body in cholera during life-time, is more readily explicable than the rise of temperature after death. The *immediate* cause of animal heat, whatever be the indirect influence of the nervous system,—which is not the less remarkable and obvious because indirect,—is the formation of carbonic acid in the general capillaries, and certain other chemical changes. There is nothing to show that nervous influence of *any* kind can produce heat independently of changes of the blood; but, as the experiments of Sir Benjamin Brodie, the effects of shock, the results of many affections of the body in which the nervous system is more or less concerned, and the physiological relations of that system with the respiration and circulation abundantly prove, we have to deal with no matter of simple chemical changes, but of changes varying with, and varied by, the complex and manifold alterations of condition which life, as a whole, is prone to undergo. All this must be granted.

There is no doubt that many affections of the nervous system may produce coldness indirectly. They act upon the heart and the respiratory movements so as to diminish the sources of warmth; but not to these must we look for the true explanation of the frigidity of cholera, which is certainly more rationally explained by reference to that condition of the blood which forbids the due occurrence of those changes without which the body must decline in temperature. The chief reason of the coldness of cholera would seem to be the non-formation of carbonic acid in the general capillaries. If this be the case, should there not be a great deficiency in the amount exhaled during the cold stage of this disease?\*

\* I am indebted to Dr. Snow for an interesting case, in which he administered oxygen. In this instance the quantity of carbonic acid exhaled was proved to be very scanty.

I believe it would have been discovered, had sufficiently exact experiments been made, that the intensity of the disease would have been nearly proportioned to the degree of diminution of the proper quantity of exhaled carbonic acid; and that an increased amount of its exhalation would have been found to denote recovery, even as it is foreshadowed by the restored secretion of bile and urine,—I mean, of course, recovery from collapse; for I forget not the coma, overwhelmed by which so many have perished, after exciting false hopes.

There seems to me, as there has seemed to others, some analogy (though I would not push it violently) between an animal with the pneumogastric nerves divided, and a patient dying of cholera. M. Provençal and Sir Benjamin Brodie found that a section of the nerves in question reduced the exhalation of carbonic acid. There is certainly less of it exhaled in cholera, far less than there ought to be.

The respiration becomes embarrassed after division of the pneumogastric; so it does in cholera. And I believe not only that it is purely *voluntary* in the former case, or nearly so, but that very often, in extreme cases of the disease in question, it is performed almost entirely by the will. In the former instance, impressions on the periphery of this nerve are useless, because its trunk is disabled from conveying them; in the latter, I believe that the periphery cannot be properly stimulated, in consequence of the condition of the blood in the pulmonary circulation; and that the *reflex* movement of respiration fails from this cause.\* Further, less oxygen is inhaled, and animal heat fails in both cases.

So far we may indulge comparison; but manifold differences separate the two conditions. In the first, the blood is affected because of the impaired respiration; in the latter, the respiration suffers and labours because of the damaged blood. Then there are the diarrhœa, the vomiting, the cramps of cholera, distressing with all the sudden and violent operation of some deadly, agonising poison. There is that strange state of blood which explains everything but its own condition,—explains the coldness, the pulselessness, the

\* It is needless to say that I adopt Dr. Marshall Hall's view of the office of the pneumogastric as an excitor of respiration.



lividity, the death,—explains also the too common and palpable failure of remedies; for how can they act when they are not absorbed,—how influence when they do not circulate? The blood's altered consistency alone would seem to explain much. When Mr. Hunter gave the reasons of the blood being fluid, he so hinted at what would happen if loss of fluidity should occur, as to draw a picture which fails not to remind us of some main symptoms of cholera:—"Without being fluid, it could not be propelled through flexible tubes, and distributed to all parts of the body; it could not be divided into portions, as the vessels branch off; it could not pass through the smaller vessels, nor admit of the various separation of its parts which are to produce the increase and repairs of the whole body; neither could it be adapted for furnishing the various secretions; nor could it be brought back to the heart."

Asking pardon for this digression, I would revert to my subject. It must not be supposed that extraordinary states of temperature after death are *peculiar* to cholera. Dr. Davy has observed some facts from which it might be surmised that the thermometer would, in certain parts of the body, demonstrate in some cases much higher degrees of post-mortem temperature than we have now any idea of. His remarks were made at the hospitals in Valetta. A man died of supposed acute rheumatism: there were found large collections of matter in the neighbourhood of the shoulder and hip-joints. The body was examined three hours and a half after death, when the temperature of the room was 86°. It was warm externally, but the deep-seated parts gave to the hand a burning sensation. The thorax and abdomen had been exposed to the air about ten minutes, when "a thermometer placed under the left ventricle of the heart rose to 113°; and under the liver, in contact with the lobulus spigelii, to 112°." What was there in this case which could explain so remarkably high a temperature? In another instance, wherein a man died suddenly, and was examined five and a half hours after death, in a room of the temperature of 86°, the thermometer placed under the left ventricle of the heart rose to 108°; placed under the liver, to 107°. Whence this great heat? In both these cases it will be seen that

the greatest warmth was declared by the thermometer being placed in contact with the *heart*; but Dr. Davy makes statements of other examinations in which there was a difference (in one case of 3°) in favour of the liver. As to the two cases which revealed so extreme a temperature, it must be well remarked that the early period at which they were examined after death would seem to set aside the notion that it was attributable to putrefaction, which, Dr. Davy says, "had not taken place in these bodies,"—"had hardly obscurely commenced." It is not known whether there was a *rise* of temperature, or only a *maintenance* of that which, *before* death, was very high. It would have been most interesting to have known the exact heat of the skin at the time of dissolution.\*

It would seem that the condition of the circulation of a part at the time of death may have much to do with its heat afterwards. M. Bouchut, in a very recent work on the signs of death, speaks of the full or empty state of the stomach in reference to the process of cooling, and cites a case placed on record by M. Ollivier d'Angers, in which the abdomen of a man, who died during digestion, was found hotter than other portions of the body. But the observation, like so many others in reference to the heat of the dead, is too vague. What was the difference in favour of the abdomen, and what was the warmest portion of this region?†

Dr. Dowler has detailed, in his "Experimental Researches," some instances of a singularly high post-mortem temperature. The following will show the temperature observed in some cases, and the sex and age of the subjects of observation:—

Sex.	Age.	Temperature.	
		Deg.	
M.	27	100	
—	30	102	
F.	26	103½	
M.	24	107	
—	25	107	to 106
—	39	107½	to 103
—	24	108	
—	22	109	to 106
—	26	109	to 102
—	50	109	to 102
—	25	111	to 102

\* See Dr. Davy's remarks on these cases, in the first volume of his Researches.

† *Traité des Signes de la Mort.* Par E. Bouchut, Ouvrage couronné par l'Institut de France. Paris, 1849. See p. 151.



The temperature was taken soon after death; but the time varied, as the reader may see by turning to the account. Sometimes an hour, or two hours, or more, elapsed before the thermometer was used. The decline of heat was frequently noticed up to a certain point. In many of the instances muscular contractility was readily made manifest by mechanical irritation of the muscles. The comparative temperature of life and death does not appear to have been fully and accurately estimated; but the subject is alluded to, and it is stated that the body of a Philadelphian, "which before death had been remarkably cold, had a temperature after death as high as  $109^{\circ}$ ." The thermometer was, in one case, applied to the chest; in another, to the axilla; but, for the most part, we are left to guess with what spot it was placed in contact. It would, however, seem that the skin was always tested, for nothing is said any where of the temperature of the internal parts. But of what diseases did the subjects of the observations die? The great majority appear to have perished of yellow fever.

The great heat observed in the dead by Dr. Dowler reminds one of the high temperature observed in birds in life-time. The following is Tiedemann's table representing the latter; and I introduce it that the reader may at once compare it with the imperfect one I have constructed with Dr. Dowler's examples:—

	Degrees.
Great titmouse . . . .	111·25
Swallow . . . . .	111·25
Fingilla, different species	111·25 to 107
Anas, different species .	111 to 106
Common hen . . . . .	109·94 to 102·99
Falco, different species .	109·74 to 104·5
Pigeon . . . . .	109·58 to 106·7
Raven . . . . .	109·23 to 105·99
Pullen . . . . .	107·49
Common cock . . . . .	103·78 to 102·99
White game . . . . .	102
Gull . . . . .	100

A very high temperature has been demonstrated in the human body during life. Even the highest ascribed to birds by Tiedemann, or the highest shown to exist in man, when dead, by Dr. Dowler, has been exceeded by the living frame. Let me allude to a remarkable case or two. The temperature of  $106^{\circ}$  has been noticed in scarlatina by Dr. Dunglison; that of  $110\cdot75^{\circ}$

in tetanus, by Dr. Prevost, of Geneva, who communicated the fact to Dr. W. F. Edwards.\* M. Piorry is said to have remarked the temperature of  $113^{\circ}$  in typhoid fever; but a still higher heat, if observed in the living body,† would, I think, be less calculated to surprise us than the amount of warmth which has been already shown to have happened in the dead. In life-time the balance between the processes whereby the frame is cooled and heated may be altered in various ways. Much may happen to favour those chemical changes whereupon animal heat more immediately depends, whilst, simultaneously, the secretion of the skin may be so checked that the surface shall operate most imperfectly as a diminisher and regulator of the warmth of the body. All parts of the subject need well tracing out, though there be writings,—and I may refer to the Memoir of M. Roger on the Temperature of Infants and Young Children,—in which the pathology of animal heat has been handled with no little ability and care. The subject, in reference to the nervous system, is of singular interest. Facts have come to light in reference to injuries of the spinal cord which compel even the attention of the incurious. M. Chossat found by experiment that division of the superior portion of this organ was the cause of a remarkable elevation of heat. Sir Benjamin Brodie noticed the temperature of  $111^{\circ}$ , both during life and directly after death, in a case of forcible separation of the fifth and sixth cervical vertebræ, giving rise to paralysis.‡ Dr. Gull lately informed me of an example of unusually high temperature succeeding to, and, no doubt, indirectly caused by, injury of the spine. Different parts of the body should, of course, be carefully tested by the thermometer in cases of this kind. Only lately I saw a man with his lower extremities entirely, his upper partially, paralysed by a large effusion of blood external to the

\* See On the Influence of Physical Agents on Life. By W. F. Edwards, M.D., F.R.S. Translated by Dr. Hodgkin and Dr. Fisher. See p. 257.

† It has been stated by Dr. Granville that the heat of the uterus has, during labour, risen so high as  $120^{\circ}$ .

‡ "I have made experiments similar to those of M. Chossat, and have met with similar results. I have also seen several cases in which an accidental injury of the spinal cord has produced the same effect."—See Sir Benjamin Brodie's Observations on Injuries of the Spinal Cord, Medico-Chirurgical Transactions, vol. xx.



dura mater of the cord: it was consequent upon fracture of two of the cervical vertebræ. At a time when the temperature of the inner surface of the lip was 97°, and that of the axilla was the same, the thermometer rose to 100° on my placing its bulb between two of the fingers or toes. Probably the state of the skin as to perspiration differing in the unparalysed and paralysed parts, may explain some cases of disparity of heat. As is known well enough, there is no *uniformity* as to the matter of temperature in cases of affection of the superior portion of the cord. Cruveilhier has given a most interesting example of spontaneous displacement of the atlas, which gave rise to compression of the cord and hemiplegia.\* Coldness of the skin, and the feel of shivering, were prominent amongst the symptoms. Cruveilhier says that the *constancy* of the coldness afforded him always fresh admiration, and even goes so far as to declare that it was so manifest and remarkable during the course of the malady as to lend support to the opinions of those physiologists who consider the spinal marrow as the main source of animal heat.

Of animal heat we have yet much to learn. Viewing it, to speak generally, as proportionate, *cæteris paribus*, to the fulness, freedom, and swiftness of the circulation, and the corresponding energy of the respiratory process, we are but ill prepared to find the high, the extremely high amount of it which has been noted in the dead. In *what* cases the heat absolutely rises after death, and in what also it rises *most*, remains to be ascertained. How can we talk of what we have not examined? According to Dr. Dowler, the rise in question is not peculiar to cholera: and it is certainly required that the more important facts which touch upon the temperature of the dead in general should be more generally and completely canvassed, for they have most interesting relations to the signs of death, whilst some of them may at some future time give birth to medico-legal inquiries, the difficulties whereof, increased alike by rumour and credulity, will alone be solved, if solved at all, by the indispensable, and but too frequently undervalued evidence of medical witnesses.

The *long delay of rigor mortis* in the case which occurred at Bristol was

certainly a very remarkable feature of it. Was it attributable, in any measure, to the warmth of the body being preserved so long? We must not be too ready to violently force *simultaneous* circumstances into the imaginary relation of cause and effect; and so commit an error which has led so often and obviously, elsewhere, to the spread of false knowledge and the discouragement of true. It happens frequently that rigor mortis lingers until the body cools; but it does not invariably happen. I have seen, as many others have done before, animals become rigid, perfectly rigid, whilst warmth remained. But there is an observation which would of itself suffice to prevent any one attributing the deferred rigidity, in the case in question, to the tardy cooling of the corpse. M. Ollivier (as Dr. Taylor has reminded us in his lectures on the signs of death), has found some bodies dead from cholera, *at once very warm and perfectly rigid*. More cases might be cited without trouble or research; but, as they are furnished not by *cholera*, they would, though perfectly applicable, seem less strikingly in point. It is very easy to conceive long retention of warmth, long maintenance of irritability, long retardation of rigor mortis, and long resistance to the changes whereupon putrefaction depends, occurring *together*; and this would seem to have been exemplified in the instance which was observed at Bristol.

There appears to be a marked disuniformity as to the time at which rigor mortis sets in after dissolution by cholera,—a circumstance to be in part, if not *altogether*, explained, by the fact, that death surprises in this disease at a time when the muscular fibre is most differently situated as to its amount of irritability. It is known well enough that the phenomenon of rigidity is generally postponed until the contractibility of the fibre is either entirely or almost exhausted. And there is certainly no more cardinal feature of this form of muscular contraction, for I cannot but regard it as such, than the fact that, though its *power* and *duration* is as a rule proportionate to the amount of irritability at the time of death, its *occurrence* should be hindered until that property be completely or all but abolished.

When we consider that cholera at-

\* Anatomie Pathologique.



tacks both the weak and the strong, that it respects no age, and that it happens sometimes in the course of maladies which have produced manifest, perhaps alarming prostration; that its duration varies not a little; that it is attended by cramps, which, to speak comparatively, are slight and partial, or universal and violent, and endure unto death;—we may safely conclude, without any further consideration, that very opposite states of muscular irritability must necessarily prevail at the moment of dissolution, and that the time at which rigor mortis will appear, the force it will be exerted with, and the period of its remaining, will all, as a consequence, be far from uniform.

The state of nutrition of the muscular tissue may be mentioned in reference to rigor mortis, not only as respects death by cholera, but other forms of it. Muscular, or, as they should be called, extraordinarily muscular persons, are of course most likely to exhibit the phenomenon in the most marked and lasting way. Atrophy, or that impaired amount of nutrition which would hardly be recognised as such, (and we do but observe this matter rudely), leads on the other hand to a less palpable and abiding form of it. The remark, as need scarcely be observed, applies equally to the involuntary as to the voluntary muscles—to the heart as to the biceps. Paralysis does not prevent rigor mortis, unless, as Sommer shows, it interferes with the nourishment of the muscles. This physiologist once remarked an entire absence of rigidity on the paralysed side, the muscles whereof appear to have been in an unhealthy state.

Fatty degeneration, (which I speak of from its commonness), has no doubt a great influence on the degree and duration of rigor mortis.\* It would too, I should think, if excessive, either lead to the prevention of cramps in life-time, or very much modify their power, for we

cannot but suppose that *cæteris paribus* the best nourished and most irritable muscles would be most liable to this peculiar form of spasmodic action. It is with the state of the living, (only in a larger, more emphatic sense) as with the condition of the dead. It is modified by many circumstances that we know, by more that we know not. How can we expect diseases to run their course in sameness, seeing that the subjects of them so manifoldly differ? How look for precisely the like states in death, perceiving clearly that at the time of dying there are many parts and actions of the body extremely differing in different persons?

Even the remedies given for the disease, may, if they have circulated, have more relation to the time of commencement of violence, and duration of rigor mortis, than might be at first supposed. I am led to the remark by the influence of chloroform, which not only, as may be shown by experiment, impairs the irritability of the muscles during life-time, but leads to unusually early rigor mortis. This I have seen myself.\* It is well known that the exhibition of chloroform has diminished, and sometimes allayed, the cramps of cholera; and it may be presumed reasonably to have accomplished this by virtue of its power to lessen, or annihilate, muscular contractility. And it is surely not extravagant to suppose that rigor mortis *might* set in somewhat earlier than common in certain cases where chloroform has been largely given, seeing that whatever impairs the irritability of muscles is favourable to their rapid post-mortem rigidity. Animals killed in the chase, being “tired to death,” have been known to stiffen almost as soon as dead. Why so? Because their irritability was utterly, or almost utterly, spent. Had they been destroyed of a sudden, during sleep, there would have been just the opposite condition of the muscular fibre, and a state wherein the postponement of rigor mortis might have been properly looked for.

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\* It would be of no little interest to examine this question thoroughly, in reference to the heart. What has been called concentric hypertrophy, by a grave pathological mistake, would, I should suppose, never be found where this degeneration is both general and extreme. I asked Dr. Quain to tell me his experience on this point. He says that hearts in such a state of degeneration are “generally found dilated, and their parietes flabby,” but that he has seen them “firmly contracted” where the degeneration was not extensive nor excessive.

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\* Dr. Tyler Smith says that he has seen it happen in the frog within ten minutes of the heart ceasing to beat. See London Journal of Medicine, Dec. 1849. It would be important to determine accurately the time at which rigor mortis is wont to occur after death by chloroform in the human subject. Where death is *suspected* to have been caused by it, the knowledge of this matter *might* perhaps much help inquiry.



The degree of swiftness with which a man dies in cholera, and the circumstance of death happening in the cold stage or not, must, of course, be considered in reference to the question of rigor mortis; nor must the fact of dissolution being apparently hurried by sudden efforts on the part of the patient, be forgotten, for death, in such a case, may be supposed to leave the muscles more irritable than they would have been, had the disease been a more lengthened course, and more exhaustion taken place, from distress, sleeplessness, and other sources of prostration ere life was past. But with regard to cholera, as well as with reference to disease in general, the state of the muscular system after death is very apt to be most cursorily touched upon, and the pathologist fancies he has done enough if he note vaguely the circumstance of rigidity, without looking to the period at which it occurred, or sufficiently and fully connecting it with the condition of the body which prevailed in life-time. And yet the phenomenon, taking simply its universality into fair account, is full of interest, but that interest is unmeasurably added to, by the diversity it puts on in conformity with differences of the muscular system in various persons, and the great dissimilarity of their mode of dying. It is a chapter in the history of death, but it forms a portion also of the history of maladies; and it is certainly deserving of a far more general and comprehensive study than it has hitherto received.

Observations have been made upon the position of limbs rigid after death by cholera, which might lead to the idea that postures of distortion are *peculiar* to this disease; but this would be an erroneous notion, though it be far from improbable, looking to the nature of cholera, the rapidity of its progress, and the strange attitudes in which its victims perish sometimes, that such postures are more commonly met with than in other affections. I was told by a nurse, whose task it was to lay out not a few who had died of cholera, that she was always careful to straighten the limbs very soon after the occurrence of death, because they were wont to stiffen very quickly, and she was then unable properly to 'compose' the corpse. M. Cruveilhier states that he has rarely seen those semi-flexed

positions which some have delineated as proper to this disease; but this might have been owing, in part at least, to those whom he examined having died in a hospital where due care was taken to arrange the bodies in due form. This pathologist, however, gives a striking example of the disordered postures whereto he makes reference. It is related by him, that an extremely vigorous man, 25 years of age, died of cholera in *seven* hours, enduring cramps to the last. Twelve hours after death, the body was rigid, and there was flexion of the legs upon the thighs, and of the latter upon the pelvis. The conjunctivæ were dark, the body livid, the eyes shrunken, and the whole aspect singularly frightful. The question to be asked clearly is—was this body "laid out?" were the limbs straightened as usual? Another question is—did any muscular contractions occur after death, and so discompose and distort the corpse? Some trifling alteration in the position of the parts may occasionally be owing to rigor mortis, and I may refer to some observations of Sommer, which bear directly on this point, but I think any physiologist of judgment would pause very long before he attributed such an attitude as this to the effect of that phenomenon; and I should not imagine that Cruveilhier, who is silent upon the matter, would think of so explaining it. The knowledge which we have of post-mortem contractions affords a far better explanation of some changes of posture occurring after death than any which could be furnished by rigor mortis. But in reference to this topic, it is indispensable to note most accurately the position at the moment of death, for, if left to itself, it will remain as it was. The common practice of "laying out" the dead, has, I need not say, reference to the acknowledged fact, that persons stiffen in the form they are left in. In various cases of violent death strange attitudes are found, well calculated to amaze the vulgar, and make yet deeper the horror of the scene. No one was present to arrange the body; the attitude of death remains. The drowned have been found in all kinds of postures. Lately I saw the arm retracted to the utmost, in a body stiffened firmly by rigor mortis. It was that of a paralytic, and had been so retracted before death. Rigor mortis may



be held to fix positions rather than make them. At the moment of my writing this, there are two frogs before me; rigor mortis has occurred long since. In one, the limbs, disposed symmetrically, lie a little apart; the other rests in a somewhat grotesque attitude. They are, with the exception of a slight flexion of the toes of one of them, exactly as I placed them before rigor mortis was established.

For myself, I have never seen a greater degree of rigor mortis after cholera than I have noted after other affections that have run their course speedily. In strong and unusually muscular subjects who die of acute diseases, it is not singular to remark the extreme firmness wherewith the limbs are stiffened, and the clear outline of many of the superficial muscles.

The involuntary muscles have sometimes been found remarkably contracted after death by cholera. The heart, the bladder, and the intestines, have all been discovered, though anything but invariably, in a marked condition of rigor mortis.

Whether or not the involuntary muscles are, to speak *generally*, most contracted (where they are so) in those cases in which rigor mortis is specially evident in such as are voluntary, I cannot say, but it is most likely that both sets of muscles would be discovered, in some instances, similarly affected to an extreme degree.

The heart has been often seen very rigid, but the rigidity of this muscle is far from universal, and in not a few instances it is quite flaccid.

The bladder has been very commonly found in extreme contraction. One writer has likened it to the unimpregnated uterus, on remarking it in this condition, from the thickness of its walls and the smallness of its cavity.

The intestinal tube has presented more or less general marks of rigor.

In the "Madras Reports" it is stated that the intestine was sometimes, but rarely, contracted; one case is mentioned in which the arch of the colon was in a state of rigor, and another in which there were several contractions of the same part.

There are some examples to be found in the "Bombay Reports."

In one case there was a contraction of the colon from the cæcum to the sigmoid

flexure, so that the sides of the intestine were brought into close contact, and it felt thick and solid; the part so influenced was not larger than the middle finger. In a second case, the colon is reported to have been "nearly obliterated," in consequence of contraction. In a third, the transverse arch of the same portion of the canal is spoken of as "almost impervious."

The Cholera Gazette for 1832 also contains facts of the like nature.

In a case examined by Mr. Pilcher, about twelve inches of ilium were found contracted; and instances are given of partial contraction of the colon.

In a post-mortem examination, made in the presence of Dr. Sims, Dr. Hope, Mr. Mayo, and Mr. Perry, the stomach was found "contracted."

Dr. Kirkes has kindly informed me of a case which occurred during the late epidemic, wherein, fifteen hours after death, "the heart was pretty firmly contracted; the urinary bladder, and large intestines, were contracted also. The limbs were exceedingly rigid, and the muscles in the most extreme degree of contraction."

But no form of rigor mortis of the involuntary muscles is *peculiar* to cholera. Rigidity of the heart is no uncommon circumstance, as every one knows; and I have seen lately some well-marked cases of it in various affections.

It was probably partial rigor of the intestines that was referred to by Harvey, famous for his great discovery, in his "Anatomical Examination of Thomas Parr," a man remembered for having lived long. "The small intestines presented several constrictions, like rings, and were muscular."\*

The following observations on rigor mortis of the involuntary muscles, to be found in the "Supplement to the Second Volume of Professor Müller's Elements of Physiology,"† may not be inappositely cited here. "The rigidity of voluntary muscles, from being the most evident, has attracted most attention, and the phenomenon has, until lately, been described solely in relation to this class of muscles; but sufficient evidence has now been accumulated to warrant the conclusion, that the invo-

\* See Works of W. Harvey, M.D., Edition of the Sydenham Society, p. 590.

† By William Baly, M.D., F.R.S., and William Senhouse Kirkes, M.D.



luntary muscles also are affected by a post-mortem rigidity, which is, in all essential respects, comparable with that seated in the voluntary muscles. And this is true, not merely in regard to those involuntary muscles which, such as the blood and lymphatic hearts, are constructed of striped fibres, but also with regard to the tissues composed of unstriped fibres, such as the contractile coat of blood-vessels, and of the large excretory ducts. The observations of Dr. George Budd and Mr. Paget have proved this in the case of the heart; and the occurrence of rigidity in the digestive canal has been shown by Valentin, who found that if a graduated tube be connected with a portion of intestine taken from a recently-slain animal, filled with water and tied at the opposite end, the water will in a few hours rise to a considerable height in the tube, owing to the contraction of the intestinal walls. The contraction of the blood-vessels after death was observed by John Hunter, and is now regarded as a well-established fact, and one by which the empty state of the arterial system after death is in a great measure explained."

In considering the *duration* of rigidity, be it in reference to cholera or not, regard must always be had to *temperature*. It is related by Nysten that Laennec showed him a squirrel which in a *cold* season was rigid seven days after death.

In a paper which I wrote, not long ago, on the muscular contractions which occasionally happen after death from cholera, I observed that in one respect (if not in more than one) they may be likened to the effects of rigor mortis, which, as may sometimes be observed of these contractions, affects parts in *succession*. This remark is far too general, and may be thought to imply more than I intended. Rigor mortis and the movements in question, have, as will be apparent to every body, many and extreme points of difference. Rigor mortis does not ensue until the irritability of the muscle is entirely or almost exhausted; but these movements are signs of irritability, sometimes of a great degree of it,—are, in some cases at least, to be *excited*, whereas nothing can excite this form of rigidity, but its one common cause. Rigor mortis influences both sides of the body equally, producing effects not less symmetrical than the

disposition of the muscles themselves, and repeats itself in various subjects, making an allowance for occasional differences, with a striking sameness; but these contractions are diversified as to form and power, affect different muscles in different subjects, cease and return repeatedly; whereas the contraction of rigor mortis being once over, is never renewed. But why proceed further?

The more we contemplate the rigidity of death, the more remarkable does its individuality appear. No other form of muscular contraction can properly be compared with it; no other so comes, so progresses, so endures, so departs. Yet such rigidity, though it be caused by a special action, which is in nowise to be explained as other kinds of action are, is quite within the pale of the admitted law, that the force of an action, no matter what its nature, is in dependence on the condition of the part performing it. Nor can it be necessary to enforce the observation by repeating at length what has been said already, as to the relation between the power and endurance of rigidity and the perfect nutrition of the muscular fibre.

Two points yet remain to be considered in reference to the case which occurred at Bristol; one relates to the retardation of putrefaction which was observed. This doubtless considerably added to the doubts of the relatives of the deceased. Putrefaction is, of course, always watched for with intense anxiety by those who are unassured as to the reality of death. Five days, in this case, passed away ere putrefaction showed itself. The time was long, notwithstanding death by an affection wherein this process tarries. But instances might be cited of its far more protracted appearance, though actual death had happened—not death-trance, a name used sometimes most improperly where dissolution was absolute, because things had happened not to take their accustomed course in the interval between the cessation of the respiration and the beginning of decay. A case occurred at Deptford, in 1844, in which a body, dead in reality, was supposed by some to be in a death-trance. It was watched long, and opinion varied as to its true condition. Putrefaction had not "far advanced" when so many as thirty-five days had elapsed after dissolution. The deceased was *young*, and had died *suddenly*. In the case at Bristol the circum-



stances were such as to make the late putrefaction no such very especial matter for surprise. The death happened in a *young* subject, was accomplished with *celerity*, and effected by a disease wherein the dead are slow to putrefy. The temperature was not high, and fresh currents of cool air were from time to time admitted into the room the body lay in. We must not construct marvels, nor magnify everything which may somewhat deviate from the course of things.

Let me here add one word further on the relation of putrefaction to the temperature of the dead. Prolonged heat and extremely tardy putrefaction have been observed *together*: but has the body been seen at the *same time* quick to putrefy and slow to cool? Dr. Taylor not long ago published in the MEDICAL GAZETTE an instructive "Case of Rapid Decomposition of the Human Body." I inquired of him how quickly it parted with its warmth, and he replied:—"Unfortunately no trustworthy history could be obtained of the cooling of the body, as it was in the hands of the nurse during the night and early part of the morning. It was quite cool when I saw it *seventeen* hours after death." It is observed by M. Deschamps, that "so long as the body preserves its natural heat there is no discoloration of the abdomen."\* It is to be observed of the case for which I am indebted to Dr. Green, that the loss of heat, the occurrence of rigidity, and the abdominal putrefaction, were nearly *coincident* marks of phenomena.

Dr. Taylor has been good enough to send me the following note of some experiments which he has been performing in reference to the question of animal putrefaction producing heat:—"I have tried experiments by inserting delicate thermometers in the midst of putrefying animal matter, but have not found the least change of temperature during the process, although the viscera were kept in glass vessels covered."† There was a thermometer outside for comparison.

It need scarcely be added, that the tendency to putrefaction is much greater after some diseases than others. The remark holds good of affections which

agree in leaving their principal traces in the same region. In cholera, as we have seen, it is later than common; but in child-bed fever it is often, as Dr. Kirkland and others have noticed, quick to appear. The same may be said of ordinary enteritis. Mr. Phillips observes of a case of intestinal obstruction:—"Although the death occurred only thirty-four hours before the examination was made, and although only sixty hours before he was apparently in good health, decomposition was greatly advanced.

"The abdomen was enormously distended; the scrotum was as large as a child's head; the surface of the trunk was covered by very large vesicles, containing a dark-coloured and very offensive fluid; and a good deal of dark bloody fluid had escaped from the mouth and nose. The cellular tissue was so distended with gas, that as soon as an incision was made through the skin it escaped with a loud hissing noise."\*

The aspect of the countenance in the Bristol case was described as placid, like that of one asleep. It probably would have excited less attention had it not been associated with retained warmth, deferred putrefaction, and delayed rigidity. It was certainly, considering the mode of death, an unusual aspect; for cholera leaves, generally, rude and deep marks of its swift, desolating course. The dead look much as the dying: there is death in the visage some time ere it comes, and the features at the moment of the last expiration seem hardly to undergo the shadow of a change. How different when some face, crimson with health perchance, turns deadly pale from sudden fatal hæmorrhage, the blood flying from the cheeks scarcely more swiftly than life from the body!†

It must not be thought that the countenance has *always* a striking haggard-

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\* In this case a portion of the ilium was strangulated, another part of the intestine looked gangrenous, and there was some dark fluid blood in the abdomen. See Medico-Chirurgical Transactions.

† A beautiful allusion, as some readers will remember, is made to the remarkable paleness which overspreads the features when death is owing to a sudden loss of blood, at the end of the tenth book of the *Æneid*. The following lines are from the touching description of the death of Lausus:—

"At verò ut vultum vidit morientis et ora,  
Ora modis Anchisiades pallentia miris:  
Ingenuit miserans graviter, dextramque  
tetendit."

\* He remarks, also, that the discoloration in question is very often coincident with cadaverous rigidity.

† Temp. 55° to 57°.



ness after death by cholera. Not only was there none in the example before us, but Cruveilhier mentions an instance in which a person who died of this disease might have been supposed to have perished from some other malady. But, generally speaking, the eyes are unusually sunken, and there is a peculiarly shrunk and ghastly countenance. Even the faces of children manifest it, though in them there is so frequently observed, after other maladies, and before the time of rigidity, a most serene, nay, to some a momentarily delusive, physiognomy.

A change has been seen in the colour of the face, which becomes less livid, but *gradually always*, as I should suppose. It sometimes puts on quite a red tint, which, like the less conspicuous change, is doubtless owing to alterations in the blood of the superficial vessels. As to the general hue, it varies much: in a case recorded by Cruveilhier the superior extremities were so livid that they at first sight seemed gangrenous to a medical observer; but in some instances the lividity is infinitely fainter, and might pass without any particular remarks.

Let us, before concluding, pass in short review some of the principal points which have been noticed by observers as to the state of bodies dead from cholera.

I. The aspect has been unusually cadaverous, though scarcely more so, in many cases, than it was in life-time, for the disease has power to make all physiognomies subordinate to its own: but the features have more than once been observed in a different state, in nowise telling of the cause of mortality.

II. The whole body has looked extremely shrunk; the hands have been very shrivelled; the distinction between the ages of corpses has not been nearly so easy as in general.

III. The colour has been dusky, often remarkably livid, and has been noticed to become lighter, and even to redden; but the hue, not changing equally, has sometimes presented a peculiar mottled appearance.

IV. Rigor mortis has set in, occasionally, with an unusual hurry; it has also been deferred extraordinarily late; has occurred when the limbs have been more or less flexed, and so fixed them; has been seen in every degree; has been noticed to influence the involuntary

muscles, the bladder very frequently, the heart not rarely, the intestines occasionally. It has presented all those varieties which might have been anticipated, seeing that the victims of the disease have been furnished by persons of every age, strength, and condition.

V. Contractions have taken place in the voluntary muscles, and been excited by percussion: they have been partial, weak, transient, or more general, stronger and long-enduring; they have varied considerably as to time of occurrence, progress, appearance, and effect; and have, for the most part, been observed in males, the most marked instances having been furnished by very muscular subjects who perished rapidly.

VI. The case which occurred at Bristol exemplifies, amongst other things, an unusually long maintenance of temperature; and there seems no doubt of many examples having happened in which the heat of the body rose considerably after death.

VII. Putrefaction was generally more delayed than usual, and has been noticed to be especially tardy. I believe it may be stated that none of the foregoing points are, strictly speaking, *peculiar* to cholera; though there was much that was indicative of the nature of the affection in the general appearance, whereof words, after all, can but give an imperfect history. The lividity, the change of colour, the varieties of rigor mortis, the lingering temperature, the rise of the same, and the muscular contractions, have all been observed in other cases than those of cholera.

I confess, when I began the consideration of the subject, that there was much in the condition of the body after cholera which, in my eyes, stood by itself. I speak not, of course, of the state of the intestines,—of what may be termed the morbid anatomy of the affection, in a peculiar sense, but of the phenomena which may be observed in the skin and muscles, the state of the temperature, and the event of contractions. But it must be admitted that we know next to nothing, from observations made at home, as to these two questions, if we except what cholera has taught us; and we cannot too moderately estimate such information as we have gained therefrom. The inquiry into the whole state of the dead, not in cholera alone, but other maladies, between the last respiration and incipient putrefaction, is in



absolute infancy.\* It needs and invites labourers. It would prove not of interest merely, but of *use*. It is called for by physiology, and especially demanded by forensic medicine; nor can medicine in general afford to forego it, since the *history* of some diseases cannot be truly called complete without it. Of course, many will ask *cui bono*—words heard commonly even in this advanced age.

As with the phenomena of cholera during life, so with others which may be witnessed after dissolution,—unless they be viewed *comparatively*, there is no chance, no hope, of appreciating them aright. We view subjects narrowly, and therefore obscurely, at first sight; not until we dwell upon them patiently, thoughtfully, do they reveal their features, and show forth their analogies. It is plain, even from the foregoing, that we ought to be well familiar with the diverse states of the body after death. In ignorance thereof we have no chance of checking absurd terrors, and of giving sound and reputable evidence on some matters of legal investigation. Novelties are ever exciting, ever popular, and such as relate to the dead, inasmuch as they more or less touch all of us, are ever likely to remain so, in a peculiar sense. Exceptional conditions will be sure to strike every one. Everybody will speculate on muscular contractions, talk of rise of temperature, and retarded putrefaction: a few will be apt to converse of “death-trances,” and to suggest, on the strength of the wildest rumours, the holding of inquests. But why this occasional ill-founded amazement? Is it, seeing the unutterable complexities of life, at all likely that the last breath should always leave the body in the same state? Perhaps we ought more to wonder than now we do that the condition of corpses is, on the whole, so uniform. I say not that this thought will weigh anything with those who have never spent ten minutes in reflecting upon life, but yet presume to speak of the vital functions as if it were the very easiest of all matters to understand them in health, and right them in disease. They expect a specific for every malady, as a matter of course; and, instead of being thankful for what medicine has accomplished, they are full of disappointment that it has done

no more. How can they, who are ignorant of the phenomena of the living, judge truly of the condition of the dead? Is not the study of life the only means whereby the nature of death can be comprehended? Amongst the things least to be feared in this country, premature burial may be named. There were rumours afloat during the past epidemic which might seem to imply some danger of it; but where was there a solitary instance which could bear inquiry? We hear of marvels in the *distance*, and begin to puzzle ourselves; but everything would be different if we were *near*. The phenomena of the case at Bristol were not the same to the medical attendants of the deceased as they were to those who knew them by report alone. I could not help observing, when the cholera was raging, that it was more dreadful to some persons to read about than to others to see. They who were in the midst of it, trying to give help, were less full of dread and discomfort than many who were safely breathing, in pleasant places, the purest air. They had no time, at any rate, for needless terrors,—no time to sit shivering at mere fictions. There are some remarks in De Foe’s “History of the Plague” which apply well to persons who take fright at nothing. Speaking of some strange rumours, he says: “But these stories had two marks of suspicion that always attended them, which caused me always to slight them, and look upon them as mere stories that people continually frightened one another with. First, that wherever it was that we heard it, they always placed the scene at the farther end of the town, opposite, or most remote from where you were to hear it: if you heard it at Whitechapel, it had happened at St. Giles’s, or at Westminster, or Holborn, or that end of the town; if you heard of it at that end of the town, then it was done in Whitechapel, or the Minories, or about Cripplegate parish; if you heard of it in the City, why then it happened in Southwark; and if you heard of it in Southwark, then it was done in the City, and the like.” Inquiry should ever precede belief; but it is a pretty general habit to make belief precede inquiry, and so all sorts of delusions are cherished and abound. Mystery has innumerable worshippers. Would that truth had so many! Men will wander in the world of shadows:

\* I may here refer the reader to some of the questions discussed by Dr. Davy in his ingenious researches.



there abuse they and exhaust their energies. But students of Nature must be severe historians, not tellers of dreams; their object is not to mystify, but to make clear—not to invent, but to discover; and they may be naturally inclined to observe—"And surely you will easily believe that we, that have so many things truly natural, which induce admiration, could in a world of

particulars deceive the senses, if we would disguise those things, and labour to make them more miraculous. But we do hate all impostures and lies, in so much as we have severely forbidden it to all our fellows, under pain of ignominy and fines, that they do not show any natural work or thing adorned or swelling, but only pure, as it is, and without all affectation of strangeness."\*

## POSTSCRIPT.

IN quoting Dr. Dowler on the subject of post-mortem temperature (see MEDICAL GAZETTE, July 5, 1850, p. 20), I have made an error which I wish to correct. I have stated that the temperature of the internal parts does not appear to have been observed; but I find, on reading the "Researches," the two following cases:—

"W. O., an Englishman, aged 27; dead five hours; neck moderately stiff; brain  $93^{\circ}$ ; epigastrium  $100^{\circ}$ ; chest  $93^{\circ}$ ; thigh  $99^{\circ}$ ; the arms strongly contractile, &c."

"J. H., an Irishman, aged 36; of gigantic frame, estimated at 200 lbs. In fifteen minutes after death the neck, jaws, and recti muscles were immovably fixed, the contractility of the arm being very powerful. In an hour and ten minutes the rigidity became universal, the thigh being at  $107^{\circ}$ . In one and a half hours the epigastrium was at  $106\frac{1}{2}^{\circ}$ , and the brain  $101^{\circ}$ ."

The following is the account of the case whereto I have referred briefly, in which a rise of temperature appears to have taken place after death:—

"J. K., a Philadelphian, aged 25; in fifteen minutes after death, presented the contractile phenomena† in their most intense form, but which declined wholly in one hour, the body being

everywhere flexible. In half an hour after, rigidity set in. This body, which before death had been remarkably cold, had a temperature after death as high as  $109^{\circ}$ , and which did not refrigerate below  $104^{\circ}$  in three hours after."

Dr. Dowler makes the following observations in reference to post-mortem heat:—"The continuance of, or rather the degree in which post-mortem heat is evolved, bears no proportion, I repeat, to the intensity of post-mortem contraction. The great heat developed in the dead body I have endeavoured to illustrate in the medical journals of our country, and will not, therefore, dwell upon that subject at present. I find, however, on examination of the original papers not yet published in detail, that for the most part, when the heat had declined, the contractility was exhausted; but that the presence of great heat, ranging as high as  $113^{\circ}$ , did not by any means imply the presence of contractility nor the absence of rigidity. Authors seem not to have been aware of the augmentation of animal heat after death. Some have, it is true, noticed an increase of heat after death from cholera, compared with the extreme coldness of the surface during the last hours of life; but has any one hinted that this post-mortem heat ever rose as high as even the healthy standard,—to say nothing of  $14^{\circ}$  or  $15^{\circ}$  beyond that?"

It is quite obvious how extremely cautious we should be in using the

\* See Experimental Researches on the Post-mortem Contractility of the Muscles, &c. By Bennet Dowler, M.D.

† I have concluded from the context that such phenomena are alluded to as may be excited by percussion.



words *rise* of temperature, as applied to the dead. The heat tested, *at the time of death*, by the thermometer in certain parts, should be compared with that manifested by the like means in the same parts at given periods afterwards. The colour of the surface, and whether it undergoes change or not, should be observed; the state of the body as to perspiration, or the absence of it, should be noticed particularly. Different parts of the surface should be compared as to heat; and the temperature of the skin and internal parts should be contrasted. The mucous membrane of the lower bowel might be examined without any incision being made.

The greater part of that which I have written on the subject, I have, in the desire to cause inquiry, written not positively, but *suggestively*. It is time that our knowledge of the alleged rise of heat in the dead should be more exact. The high temperature noticed by Dr. Dowler some time after death is of great interest; and, though it were shown to be the remains of heat gene-

rated in life-time, it would be so still. The highest temperature which I have observed in the dead is  $104^{\circ}$ . The observation was made on the axilla of a man who had died of apoplexy. There were well-marked muscular contractions seen also: I hope to detail them in a future contribution.

I find the parts tested by the thermometer by Dr. Dowler have been more frequently specified than I supposed. And I must conclude by the remark, that, though I differ from this writer in many of his conclusions, and dissent from the criticism in which he has indulged, the phenomena he has narrated are extremely well worthy the close attention of physiologists. Mr. Helps has already repeated Dr. Dowler's remark on the ease with which post-mortem contraction may be excited by *percussion*; and a careful inquiry into the condition of the dead as to the state of their temperature, might be found to confirm the accuracy of the observations, if not the correctness of the inferences, of this inquirer.





